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change of opinion on the part of those in whose decisions on the matter we have full confidence.

It is quite impossible for congress, when it grants an immunity to colleges in the importation of printed matter duty-free, to set forth in detail the administrative processes which are necessary to secure its purpose. Congress acts on the assumption that the executive departments of government have wisdom enough in so ordering details, that the purpose of congress shall be adhered to, and that education shall have the advantages the people, through them, have decreed. Everybody but an executive routinist, whose perceptions are dwarfed by his habit, sees a higher claim in the spirit than in the letter of a law. It were a libel on barbarism to stigmatize as barbaric the recent decision of the treasury, which requires twelve oaths a year and attendant time and money for a monthly periodical to secure a free entry. Let us commend to the astute revenue-officials the story of Poor Richard and the barrel of salt beef, when a single grace over the whole could save for twelve-months' dinners a considerable fraction of the time allotted to the poor dwellers of the globe. Further let them remember graces at dinner do not cost notary's and justice's fees.

LETTERS TO THE EDITOR.

*** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

The Ohio earthquake.

A slight earthquake was felt here at 2 h. 43 m. this afternoon. Hanging lamps were made to vibrate, and at one of the public-school buildings a panic occurred among the children. The shock was not noticed by those who were busily employed at the time. No attempt was made to measure its direction or force.

E. T. NELSON.

Delaware, Ohio, Sept. 19.

The steep slopes of the western loess.

In Mr. Macfarlane's paper on the formation of cañons and precipices (*Science* for Aug. 1), there is a discussion of the cause of the steepness and permanence of the slopes in the loess region of the west. The fact is certainly a striking one. But Mr. Macfarlane's explanation, likening it to 'a well-built piece of miniature natural earth masonry well bound together,' scarcely does justice to the subject. For, in the first

place, the steep slopes recur in the typical loess, even *after it has been moved and worked over*; especially after it has lain for a few years, so that a slight 'binding-together' of the particles by calcic carbonate is renewed. In the second place, the form of the loess particles is, as a rule, not flattened, but roundish; as can readily be seen, when the sediments from a mechanical analysis of the material are examined. But this general roundness of the particles is accompanied by an extreme roughness of surface, precisely such as is seen on the large scale in the 'loess puppets' themselves. The entire mass, in fact, consists of small calcareous secretions, with rough concretionary surface, intermingled with a comparatively small proportion of fine dust and clay (see *Amer. Journ. Sc.*, n. s., vii. 10); and, when treated with dilute acid, the whole frequently becomes altogether impalpable. These rough concretionary sand-grains naturally can move only with great friction in the mass; and the latter being, moreover, very porous, absorbing instantly even a copious shower, there is little opportunity for washing away. Aside from these purely physical causes, the rapid formation of a tissue of cryptogamic fibrils and gummy matter (mostly moss prothallia) on the fertile material, soon binds the surface, and imparts additional stability.

E. W. HILGARD.

Berkeley, Cal., Aug. 20.

An open polar sea.

In an article in the New-York *Herald* of Sept. 10, Joseph W. Cremin, A.M., comments upon some remarks made by me before the British association at Montreal, in regard to the theory of an open polar sea. Mr. Cremin agrees with Lieut. Greely in the belief that there is such a sea, but fails to put forward any facts in support of his theory. And in view of the fact that so far we have found nothing but ice along the southern border of this unknown region, it is fair to presume that the ice-cap extends over the pole, unless facts can be brought forward to prove to the contrary.

Now, the facts that have convinced me that there is no permanent open water are these: 1°. Migratory birds do not pass into this region beyond the highest known land; and there is a decrease of animal life as you go north, both in the sea and on the land. Also the annual mean temperature falls as you approach the pole. 2°. The ancient ice which is being constantly displaced by the new ice that forms in the cracks opened by tides and gales is constantly coming down from the higher latitudes. If there were an open sea to the north, would this be the case? It naturally yields toward the side of the least resistance. 3°. The water in the Arctic Ocean stands at a temperature of +29° F. from October until June, with a range of less than .3 of 1°. Off the northern coast of North America, the currents are variable; and if there were an open sea, which must necessarily be a warm sea, around the pole, we should have a variable temperature in the sea-water. 4°. There is less than 1,300 miles of this unexplored region on a line drawn from Lockwood's highest, over the pole, to North-east cape, Siberia. Now, if there were a sea of warm water in this comparatively small space, we should have in the region surrounding it a meteorological condition which does not exist. We should have a vast amount of precipitation during the winter, with cloudy weather; instead of the clear dry weather, with frequent calms, that we do experience. And the amount of precipitation decreases as you go north.

The difference in temperature between the flood

and ebb tide, noticed by Lieut. Greely, I think is explained by the fact, that, to the south of Robeson Channel, the sound is kept open more or less by a strong current, and the water so exposed loses more of its latent heat in the winter than that to the north where it is protected by the ice-cap; and as the difference was only about .2 of 1°, it may be there is a difference of density. As to Mr. Cremin's theory that the flattening of the earth at the poles brings the outer crust nearer to the internal fires of the earth, I can only say that I know it to be a fact that the surface indications within the arctic circle do not bear him out in his theory. As it is a well-known fact that the earth north of the arctic circle is perpetually frozen to a great depth, and as the earth probably cooled from the surface, it is fair to presume that it at least cooled as fast at the poles as at the equator; and I think that a residence of a year or two will convince any reasonable man that the crust is tolerably thick up there, if extreme cold has any thing to do with it.

P. H. RAY.

Washington, Sept. 13.

Discrediting American science.

On p. 48 of the current volume of *Science* you take occasion to say,—

"Work of value upon the subject of micro-organisms is not done in this country, nor will it be until some such encouragement is offered to investigators as is the case in France and Germany. This kind of research requires the rare combination of many forms of training, added to a critical, analytical, and judicial mind. These we can have; but until the facilities for work are offered, until the necessity for personal sacrifice and self-denial is done away with, we can hope for no better work in the future than has been done in the past: in other words, what is first needed in order to place our own investigations upon an equality with those of the two countries mentioned above, is a thoroughly equipped, fully endowed laboratory, with a strong corps of well-trained and salaried officials."

Now, while you doubtless had in mind, when penning this paragraph, the great desirability of more systematic investigation in this country of those plagues of mankind which annually cut short so many valuable lives, I cannot allow this sweeping and unjust assertion to pass unnoticed, and to stand as a disparagement to American science and a reproach to American investigators. Whether you realize it or not, it is nevertheless a fact, that the patient student of micro-organisms in this country has been laboring under the enormous disadvantage that his work, however valuable it may be, is discredited at home, and unnoticed abroad, while the most absurd generalizations of the European worker are received with approval there, and enthusiasm here.

Sternberg has worked for years on intermittent fever, tuberculosis, septicaemia, yellow-fever, germicides and allied subjects; and, beyond his own writings and the reviews of his books, what is there in American literature to show that such a man has existed? About the time that Pasteur announced the discovery of his now celebrated 'new disease' produced by inoculating rabbits with the saliva of a child dead of hydrophobia, Sternberg demonstrated the virulence of normal human saliva when rabbits were inoculated with it.¹ He also demonstrated beyond question that this was due to a micrococcus which might be cultivated to the eighth culture without losing its virulence, and even showed that an immunity might be

granted by protective inoculation.² Both had been working at the same time with the same organism, and had reached substantially the same result. Pasteur's work was published as something remarkable the world over; while Sternberg's—well, we must admit it received some credit abroad, even if it fell flat at home. Again: Sternberg's tests of germicides are, perhaps, the most extensive and satisfactory investigations in this line that have ever been made. He was certainly one of the first who attempted to obtain exact results by allowing a disinfectant of a given strength to act on a particular disease-germ for a given length of time, and then tested his results by cultivation and inoculation experiments.³ And surely his experiments and results in photographing micro-organisms cannot be set down as entirely valueless.³

A short time ago the rather absurd speculations of Tyndall, in regard to the nature of the immunity from contagious diseases which is conferred by a previous attack, attracted wide-spread attention both in Europe and America. Tyndall's views were based upon the theories of Pasteur; and these, in turn, rested upon a very narrow basis of experimentation with fowl-cholera, which, at the time they were put forth, were far-fetched, and now are antiquated. Pasteur is a chemist, and Tyndall a physicist; and neither has any adequate conception of the fact that there are processes going on in the animal body which both chemistry and physics are incompetent to explain. Pasteur's chemical explanation of the mystery of immunity—that it was the exhaustion from the body of something necessary for the nutrition of the virulent germ; something that, once exhausted, was not again replaced—had a great fascination for the great English physicist, and he received it with childlike trust. What objection could there be, indeed, from his stand-point, to the view that a living body may be compared in every respect with the test-tube and the flask with which he is in the habit of experimenting in his laboratory? And when a Frenchman and an Englishman unite in pressing so plausible a theory, we surely could hardly expect from past experience that the American scientific editor would pay much attention to the vulgar home worker, no matter how striking his experiments, or how conclusive his demonstrations. I trust, however, you will pardon me for calling your attention to the fact that more than two years ago I demonstrated that immunity was only relative, and never absolute; that the most susceptible individual possessed a certain degree of immunity which can be accurately measured; and that all degrees of immunity may be overcome by a sufficient increase in the dose of virus. The immunity of the animal body, then, in no sense resembles the exhausted cultivation-liquid in the flasks of Pasteur and Tyndall, which no increase in the amount of virulent material added can ever induce to support the development of new generations of the microbe; and the honor of demonstrating this radical difference is due to American investigations.

I went farther than this, however, and showed that this theory of our European friends was absolutely untenable; because broth made with distilled water from the flesh of an animal that had been granted a very complete immunity was just as favorable a medium for the growth of the virus as that made from

¹ Bacteria. By Dr. ANTOINE MAGNIN and GEORGE M. STERNBERG, M.D., F.R.M.S. New York, *William Wood & Co.*, 1884, pp. 355-376.

² *Ibid.*, pp. 209-225. National board of health bulletin, July 23, 1884.

³ Photo-micrographs, etc. By GEORGE M. STERNBERG, M.D. New York, *William Wood & Co.*, 1884.